gist finds it harder to make comprehensible to the laity, than that a frog, as a complete animal, may be killed by destruction of its nervous system, yet most of its organs remain alive for hours; also the fact that it is not only possible in many cases to isolate particular organs or cells, keeping them alive for study after killing the rest of the plant or animal, but that this is even necessary, if the working of any complex organism is to be really understood. This popular ignorance, like all ignorance, has evil results. Much of the disquietude which many persons now feel in regard to physiological experiment is due to the fact that they do not realize that experiments on living hearts or muscles are usually carried out on animals which, as a whole, have previously been killed by destruction of the brain.

The remarkable operation so successfully performed by Dr. William Fluhrer of New York, involving no less a difficulty than the probing of the brain-substance itself in search of an embedded bullet, and the extraction of the missile through a counter-opening in the skull opposite the point of entrance, marks a new step in surgery which is startling in its suggestiveness. It could hardly have been anticipated that so complete a recovery would follow an operation of such difficulty and danger, involving as it did the retention in the brain, for a prolonged period of time, of a rubber drainage-tube passing completely through the head from the forehead to the back of the skull. The recovery is more remarkable on account of the additional complication of a severed artery which could not be tied, and which threatened speedy death from hemorrhage. The case illustrates the value of antiseptic or aseptic treatment, as well as the possibility of removing much brain-tissue in man, with thus far relatively little damage, which had already been demonstrated for other animals, notably for the dog. This had, however, been fairly well established for man in some cases of injury, where the surgeon had hesitated to interfere very actively. An account of this remarkable case will be found on another page. While its success would appear to justify a similar procedure under like circumstances, it is still far from certain that the next case would prove so easy of operation.

LETTERS TO THE EDITOR.

Mental capacity of an infant.

Apropos of 'Acquisition in infants,' I am tempted to state the results of an experiment I made, not long since, to test the mental capacity of Helen R. H., on the day she was fifteen months old, walking actively, but speaking only half a dozen words.

With pencil and paper, and several reliable witnesses present, I sat down, and without making any signs, or allowing signs made by others, the mother and I began to give the child a series of commands, the execution of which involved an accurate knowledge of various verbs, nouns, and pronouns. The commands were given distinctly, very seldom repeated, and were obeyed very promptly, without any questioning or explanation whatever. In one hour's time sixty-one commands were obeyed by the child with absolute precision, which showed a remembrance and correct understanding of thirty-one verbs and fifty-one nouns and pronouns. The commands given were such as the following: 'Kiss your hand,' 'Make a bow,' 'Knock on the door,' 'Blow out the candle,' 'Put the basket on the pail,' 'Put the pan in the pail,' 'Bring the bell, ball, orange,' etc. The words used were such as the child had acquired a knowledge of by observation chiefly; for not one-fourth of them had ever been taught her. I will add, that, while the child is possessed of wholesome brightness and intelligence, she has never been thought precocious. W. T. H.

Nutritive value of cellulose.

In giving an account of some recent experiments upon the digestibility of cellulose by herbivorous animals (*Science*, No. 100, p. 11), the writer took occasion to point out that the conclusions which certain writers had drawn from these experiments, regarding the nutritive value of digestible cellulose, were not sustained by the facts.

The last number of the *Zeitschrift für biologie* (xxi. 67) contains a paper by W. v. Knieriem upon the utilization of cellulose in the animal organism, in which are detailed experiments upon the digestibility of cellulose, and upon its nutritive effect, which strikingly corroborate the belief above mentioned.

The method of experiment adopted is a novel one. It consisted in feeding the animals (usually rabbits) with food containing no cellulose; the necessary bulk being supplied by horn-shavings, which were usually eaten freely, and which, as special experiments showed, were entirely unacted upon in the alimentary canal. After all the cellulose of the previous feeding had thus been removed from the animal, either a fodder containing a known amount of cellulose, or some more or less pure form of cellulose itself, was introduced into the ration. The solid excrements were collected and analyzed in the usual way, and, by means of a return to the original cellulose-free ration, all the indigestible cellulose was finally eliminated from the body.

The digestion experiments offer nothing of special interest in this connection, and we pass at once to the experiments upon the nutritive value of the digested cellulose. These were so arranged as to compare the effect of the latter with that of an equal weight of sugar in two respects: 1°, as to its influence upon the proteid metabolism of the body; and, 2°, as to its influence upon the gain or loss of fat.

The influence of carbhydrates in the food, as is well known, is to decrease the proteid metabolism, as is shown by the diminished excretion of nitrogen in the urine. In v. Knieriem's experiments, 22 grams of crude fibre, of which 11.02 grams were digested, decreased the proteid metabolism by 22%, while 11 grams of cane-sugar decreased it 15.3%: in other words, the digestible crude fibre showed itself more effective in this respect than an equal weight of sugar.

As regards the gain or loss of fat, the advantage is on the side of the sugar; the latter diminishing the daily loss from the body by 2.5 grams, while the cellulose decreased it by 1.7 grams.

These are the results of a single experiment, and, as regards exact numerical values, are of course subject to correction by future investigations. They certainly show, however, that the nutritive value of cellulose is by no means insignificant, and probably not very much below that of other carbhydrates. If, as in the former article, we assume that the heat evolved by the fermentation of the cellulose in the alimentary canal is of use to the organism, then the sole loss by the fermentation is the latent energy carried off in the marsh-gas evolved. In that paper the amount of that loss was estimated on the basis of Henneberg and Stohmann's determinations of the amount of marsh-gas excreted in their respiration experiments. If, instead of this, the amount of marsh-gas evolved in the fermentation of one gram of cellulose be made the basis of the calculation, a somewhat lower value for the cellulose results. According to Tappeiner, one gram of cellulose yields 0.335 grams CO_2, 0.047 grams CH_4, and 0.618 grams of organic acids. One gram of cellulose yields 4,452 cal.; 0.047 grams CH_4, 614 cal.: leaving 3,838 cal. to represent the available heat-value of the cellulose. One gram of cane-sugar yields 4,173 cal.; one gram of starch, 4,479 cal.: consequently, if our fundamental assumption is correct, the value of one gram of cellulose is about 92% of that of cane-sugar, and about 86% of that of starch. These results agree well with those of v. Knieriem's experiments; and the two together appear to justify the conclusion, previously stated, that the nutritive value of cellulose is not greatly inferior to that of other carbhydrates.

H. P. ARMSBY.

The naval observatory publications.

Referring to your criticism in *Science* for April 3, on the delay in printing annual volumes of 'Astronomical and meteorological observations' made at the U. S. naval observatory, I am glad to be able to say that the cause for complaint in this direction has been, at least temporarily, removed; and in future we hope to have our volumes printed as fast as the limited number of computers will permit the proof-sheets to be sent to the printer.

During the closing days of congress, the following resolution was introduced and concurred in: "That the annual volume of the 'Astronomical and meteorological observations' of the naval observatory for the years 1881 and 1882 be printed, and that 2,000 additional copies of each volume be printed, of which 400 copies will be for the use of the senate, 800 for the use of the house, and 800 for the use of the navy department, or for sale at the cost of paper and printing."

The manuscript sheets of the volume for 1881 are now in the hands of the printer, to be followed immediately by those for 1882; so that both of these volumes will be distributed this year, and it is hoped that we will continue to be able to have (as you very pertinently suggest) all annual volumes printed independently of the regular appropriation for the navy department.

ALLAN D. BROWN,
Commander, U. S. navy.

U. S. naval observatory, Washington, D.C.,
April 6.

An attempt to photograph the corona.

Mr. Pickering's interesting experiments described in *Science* for April 3 would seem to be practically conclusive as to the unreality of the coronal forms which appear upon the plates of Dr. Huggins and Mr. Woods, if it were evident that he had observed all the conditions which they indicate as essential.

His letter, however, is silent in respect to one important point. It is not stated whether or not the plates were 'backed' with any light-absorbing substance, in order to prevent the so-called 'halation' produced by reflection from the back surface of the plate under a strong light. The English observers insist urgently upon the necessity of this precaution, and use for the purpose, I believe, a coat of asphalt varnish, colored with Brunswick black. It is possible that even this expedient would not wholly prevent a streaky scattering of light at the edge of the sun's image, because minute particles of foreign matter embedded in the glass itself would have their influence; but it is obvious, that, if the experiment was tried without the precaution, it cannot be looked upon as any way decisive.

Perhaps Mr. Pickering would kindly supplement his communication by a brief statement regarding this point.

C. A. YOUNG.

Princeton, N.J., April 8.

In reply to Professor Young's communication, I would say that the precaution to which he refers was carefully attended to, and that all the plates employed were backed the day before the eclipse with asphalt varnish. It would be very interesting to know how far the corona, as photographed by Dr. Huggins, extends from the sun: for a very long exposure would probably mask the real phenomenon; one that was very short would be insufficient to obtain an impression of it. My exposures were so timed, that, by a long development, the darkening could be traced to a distance of .8 of the sun's diameter, while, with a short development, the darkening only reached to .2. But in no case could any particular rays be identified in the different photographs.

WM. H. PICKERING.

Sir William Thomson's Molecular dynamics.

As it is possible that some of your readers may have obtained copies of the papyrograph report of my lectures on molecular dynamics, delivered at Baltimore during October, 1884, I should be obliged by your giving publicity to the following corrections: —

P. 34, lines 18 and 19, delete 'We may call it a dynamax but not a paradox.' I have no recollection of, nor can I imagine, what the word was that I suggested as more logical than 'paradox.'